on mixing with air and coming in contact with a flame, or an incandescent body, the hydrogen sulphide would be oxidised, and resolved in sulphur and water (with the production of small quantities of sulphur dioxide); the sulphur, minutely divided, would remain long suspended in air, and cause the condensation to cloudy consistency of the aqueous vapour. Piria illustrated his explanation by a simple experiment: if in a vessel containing a mixture of sulphuretted hydrogen and air a lighted taper is introduced, a dense mist is rapidly formed; a similar mist is produced when glowing charcoal, or highly heated lava, or pumice, or glass, or red-hot iron is introduced in the gaseous mixture. When there is a large proportion of H_2S, the oxidation is very rapid, and the mixture explodes and burns.

Piria's explanation cannot be applied to the *Bocca della Solfatara*, where the presence of H_2S cannot be detected either by the sense of smell, or by the lead-acetate test-papers. In the "Memorie Geologiche sulla Campania (*Rendiconti della Reale Accademia delle Scienze di Napoli*, 1849, p. 137) Prof. A. Scacchi, after having opposed Piria's opinion, gives the following explanation: "I believe the increase of the vapoury cloud due to the carbonic acid produced in the combustion of the tinder, its affinity for water causing the precipitation of the invisible vapour, and thus producing a mist." According to Prof. Scacchi, in the presence of large quantities of aqueous vapour, and at the temperature of the fumarole, carbonic dioxide would act as hydrochloric acid gas which fumes in ordinary air.

Since 1849 no one (as far as I have gathered) has suggested any new opinion or tried some experiment to explain the phenomenon in question. I thought it would be interesting to test experimentally at the Solfatara the opinion of Prof. Scacchi. I was inclined to believe that, if at the ordinary temperature carbonic dioxide does not condense aqueous vapour from the air, there was very little probability that the condensation would be caused at temperatures as high as those of the vapours issuing from the Bocca of the Solfatara (about 90° Centigrade externally); the action of flames or smouldering bodies in augmenting the vapoury cloud appeared to me as chiefly due to the condensation around the minute particles of soot or dust produced during the combustion.

The following experiments were done during a clear day, when abundant vapours were issuing from the large fumarole:—

1. A Wolff bottle (1 litre capacity), from which a constant current of carbon dioxide was obtained (by pouring dilute hydrochloric acid on marble fragments), was placed on the ground inside the fumarole. The cloud of vapour augmented.

2. By means of a caoutchouc tube the CO_2 from the generator was conducted near the hottest invisible vapour. This vapour became interspersed with cloudlets of condensed vapour, and the cloudy pillar outside the Bocca greatly augmented.

3. A large bottle (of about 15 litres capacity) filled with carbon dioxide was brought inside the cavity, and the CO_2 poured out. The effect was most striking outside by the voluminous, but not immediate, outbursts of cloudy vapour.

4. With bellows of the kind used for sulphuring vines, I blew sulphur dust inside the cavity. This caused the production of great volumes of visible vapour. The same effects were produced every time that minutely divided bodies (wheaten flour, oxide of magnesia, chalky dust, &c.) were blown, or thrown, inside the cavity or near the invisible vapour.

5. The effect was very striking when the action of the carbon dioxide (from the Wolff bottle) was combined with the action of the sulphur dust.

6. A small alcohol flame augmented the cloudiness of the vapour.

7. The smoky flame of burning naphthalene acted much more powerfully than the alcohol flame.

From these experiments, which (with the exception of 3 and 6) were often repeated, the following conclusions may be drawn:—

1. Carbon dioxide helps to condense watery vapour.
2. Minute bodies suspended in air are a powerful cause (the principal cause, as Coulier and Aitken have shown) in the condensation of aqueous vapour.
3. The action of flames, or of incandescent bodies, in augmenting so remarkably the volumes of visible vapour rising from the fumaroles of the Solfatara must be ascribed both to the carbon dioxide and to the minute carbonaceous particles set free during the combustion.

Of these conclusions the first requires to be confirmed by careful laboratory experiments. ITALO GIGLIOLI

Laboratory of Agricultural Chemistry,
R. Agric. College, Portici

STATE OF THE ATMOSPHERE WHICH PRODUCES THE FORMS OF MIRAGE OBSERVED BY VINCE AND BY SCORESBY

IN 1881, when I wrote the article *Light* for the *Encyc. Britt.*, I had not been able to meet with any detailed calculations as to the probable state of the atmosphere when multiple images are seen of objects situated near the horizon. I had consulted many papers containing what are called "general" explanations of the phenomena, but had found no proof that the requisite conditions could exist in nature:—except perhaps in the case of the ordinary mirage of the desert, where it is obvious that very considerable temperature-differences may occur in the air within a few feet of the ground. But this form of mirage i essentially unsteady, for it involves an unstable state of equilibrium of the air. In many of Scoresby's observations, especially that of the solitary inverted image of his father's ship (then thirty miles distant, and of course far below the horizon), the *details* of the image could be clearly seen with a telescope, showing that the air must have been in equilibrium. The problem seemed to be one well fitted for treatment as a simple example of the application of Hamilton's *General Method in Optics*, and as such I discussed it. The details of my investigation were communicated in the end of that year to the Royal Society of Edinburgh, and will, I hope, soon be published. The paper itself is too technical for the general reader, so that I shall here attempt to give a sketch of its contents in a more popular form. But a curious little historical statement must be premised.

It was not until my calculations were finished that I found a chance reference to a great paper by Wollaston (*Phil. Trans.* 1800). I had till then known only of Wollaston's well-known experiment with layers of different liquids in a small vessel. But these, I saw, could not reproduce the proper mirage phenomena, as the rays necessarily enter and emerge from the transition strata by their *ends* and not by their lower *sides*. This experiment is by no means one of the best things in Wollaston's paper, so far at least as the immediate object of the paper is concerned. That so much has been written on the subject of mirage during the present century, with only a casual reference or two to this paper, is most surprising. It may perhaps be accounted for by the fact that Wollaston does not appear to have had sufficient confidence in his own results to refrain from attempting, towards the end of his paper, a totally different (and untenable) hypothesis, based on the effects of aqueous vapour. Be the cause what it may, there can be no doubt that the following words of Gilbert were amply justified when they were written, early in the present century:—"In der That ist Wollaston der Erste und Einzige, der die *Spieglung aufwärts* mit Glück zu erklären unternommen hat." For his methods are, in principle, perfectly correct and suffi-

ciently comprehensive; while some of his experiments imitate closely the state of the air requisite for the production of Vince's phenomena. Had Wollaston only felt the necessary confidence in his own theory, he could hardly have failed to recognise that what he produced by the extreme rates of change of temperature in the small air-space close to a red-hot bar of metal, could be produced by natural rates of change in some ten or twenty miles of the atmosphere:—and he would have deserved the credit of having completely solved the problem.

Six months after my paper was read, another happy chance led me to seek for a voluminous paper by Biot, of which I had seen no mention whatever in any of the books I had previously consulted. The probable reason for the oblivion into which this treatise seems to have fallen is a curious one. It forms a considerable part of the volume for 1809 of the *Mém. de l'Institut.* But in the three first great libraries which I consulted, I found this volume to be devoid of plates. In all respects but this, each of the sets of this valuable series appeared to be complete. Without the figures, which amount to no less than sixty-three, it is practically impossible to understand the details of Biot's paper. The paper was, however, issued as a separate volume, "*Récherches sur les Réfractions extraordinaires qui ont lieu près de l'horizon*" (Paris, 1810), which contains the plates, and which I obtained at last from the Cambridge University Library. I have since been able to procure a copy for the Edinburgh University Library. Biot's work is an almost exhaustive one, and I found in it a great number of the results which follow almost intuitively from my methods:—such as the possible occurrence of *four* images, under the conditions usually assumed for the explanation of the ordinary mirage; the effects of (unusual) refraction on the apparent form of the setting sun; &c. But it seems to me that Biot's long-continued observations of the phenomena as produced over extensive surfaces of level sand at Dunkirk have led him to take a somewhat onesided view of the general question. And, in particular, I think that his attempted explanation of Vince's observations (so far as I am able to understand it; for it is very long, and in parts extremely obscure and difficult, besides containing some singular physical errors) is not satisfactory. His general treatment of the whole question is based to a great extent upon the properties of caustics, though he mentions (as the *courbe des minima*) the "locus of vertices" which I had employed in my investigations, and which I think greatly preferable. There can be no doubt, however, that Biot's paper comes at least next in point of importance to that of Wollaston:—though in his opinion Wollaston's work was complete only on the physical side of the problem. "Sous le rapport de la physique son travail ne laisse rien à désirer."

But, if the chief theoretical papers on the subject have thus strangely been allowed to drop out of notice, the case is quite different with several of those which deal with the observed phenomena. Scoresby's *Greenland*, his *Arctic Regions*, and his *Voyage to the Northern Whale Fishery*, are still standard works; and in them, as well as in vols. ix. and xi. of the *Trans. R.S.E.*, he has given numerous careful drawings of these most singular appearances. The explanatory text is also peculiarly full and clear, giving all that a careful observer could have been expected to record. It is otherwise with the descriptions and illustrations in Vince's paper (*Phil. Trans.* 1799). In fact the latter are obviously not meant as *drawings* of what was seen; but as *diagrams* which exhibit merely the general features, such as the relative position and magnitude of the images:—the details being filled in at the option of the engraver. That such was the view taken by Brewster, is obvious from the illustrations in his *Optics* (*Library of Useful Knowledge*), for while one of Scoresby's drawings is there *copied*, one of Vince's is treated in a highly imaginative style by the reproducer.

Scoresby's sketches are composite, as he takes care to tell the reader, so that in the reproduction below (Fig. 1) I have simply selected a few of the more remarkable portions which bear on the questions to be discussed. It is to be

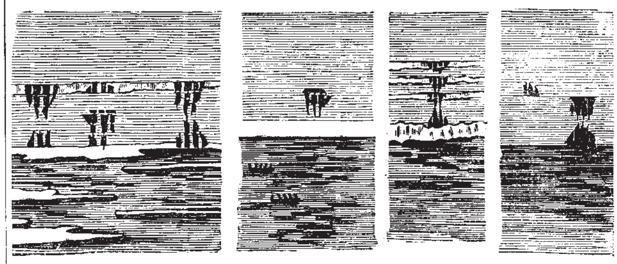

Fig. 1.

remarked that the angular dimensions of these phenomena are always of *telescopic* magnitude:—the utmost elevation of an image rarely exceeding a quarter or a third of a degree.

Because the rays concerned are all so nearly horizontal, and (on the whole) *concave* towards the earth; and because they must also have on the whole considerably greater curvature than the corresponding part of the earth's surface, especially if they happen to have points of contrary flexure; it is clear that, for a preliminary investigation, we may treat the problem as if the earth were a plane. This simplifies matters very considerably, so that definite numerical results are easily obtained; and there is no difficulty in afterwards introducing the (comparatively slight) corrections due to the earth's curvature. But these will not be farther alluded to here.

Of course I began, as almost every other person who has thought of the production of the ordinary mirage of the desert must naturally have begun, by considering the well-known problem of the paths of projectiles discharged from the same gun, with the same speed but at different elevations of the piece. This corresponds, in the optical problem, to the motion of light in a medium the square of whose refractive index is proportional to the distance from a given horizontal plane. Instead, however, of thinking chiefly of the different elevations corresponding to a given range, I sought for a simple criterion which should enable me to decide (in the optical application) whether the image formed would, in any particular case, be a direct or an inverted one. And this, I saw at once, could be obtained, along with the number and positions of the images, by a study of the form of the locus on which lie the *vertices* of all the rays issuing from a given point. Thus, in the ballistic problem, the locus of the vertices of all the paths from a given point, with different elevations but in the same vertical plane, is an ellipse.

Its minor axis is vertical, the lower end being at the gun; and the major axis (which is twice as long) is in the plane of projection. Now, while the inclination of the piece to the horizon is less than 45°, the vertex of the path is in the *lower* half of this ellipse, where the tangent leans forward from the gun; and in this case a small increase of elevation *lengthens* the range, so that the two paths do not intersect again above the horizon. In the optical problem this corresponds to an *erect* image. But, when the elevation of the piece is greater than 45°, the vertex of the path lies in the *upper* half of the ellipse, where the tangent leans back over the gun; and a small increase of elevation *shortens* the range. Two contiguous paths, therefore, intersect one another again above the horizon. And, in the optical problem, this corresponds to an *inverted* image. In symbols, if the eye be taken as origin and the axis of x horizontal, there will be a direct image for a ray at whose vertex dy/dx and x (in the curve of vertices) have the *same* sign, an inverted image when the signs are different.

Hence, whatever be the law of refractive index of the air, provided only it be the same at the same distance from the earth's surface, (*i.e.* the surfaces of equal density parallel planes, and therefore the rays each symmetrical about a vertical axis) all we have to do, in order to find the various possible images of an object at the same level as the eye, is to *draw the curve of vertices for all rays passing through the eye, in the vertical plane containing the eye and the object, and find its intersections with the vertical line midway between the eye and the object.* As soon as this simple idea occurred to me, I saw that it was the very kernel of the matter, and that all the rest would be mere detail of calculation from particular hypotheses. Each of the intersections in question is the vertex of a ray by which the object can be seen, and the corresponding image will be erect or inverted, according as the curve of vertices leans from or towards the eye at the intersection. Thus, in Fig. 2, let E be the eye, and

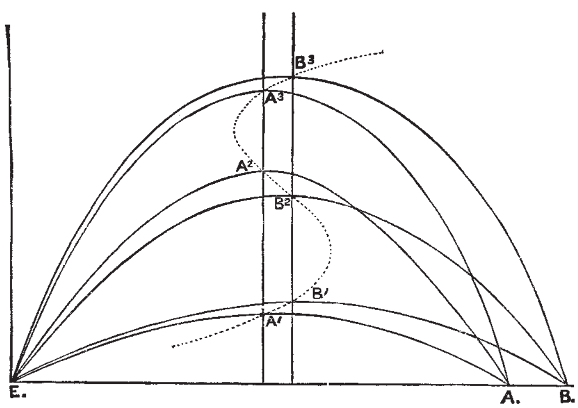

FIG. 2.

the dotted line the curve of vertices for all rays in the plane of the paper, and passing through E. Let A be an object at the level of the eye, $A^1 A^2 A^3$ the vertical line midway between E and A. Then A^1, A^2, A^3 are the vertices of the various rays by which A can be seen. If we make the same construction for a point B, near to A, we find that whereas the contiguous rays through A^1, B^1 and through A^3, B^3 do not intersect, those through A^2, B^2 do intersect. At A^1 and A^3 the curve of vertices leans *from* the eye, and we have erect images; at A^2 it leans back *towards* the eye, and we have an inverted image. And thus, if this curve be continuous, the images will be alternately erect and inverted. The sketch above is essentially the same as one given by Vince, only that he does not employ the curve of vertices. If the object and eye be not at the same level, the construction is not *quite* so simple. We must now draw a curve of vertices for rays passing through the eye, and another for rays passing through the object. Their intersections give all the possible vertices. (This construction of course gives the same result as the former, when object and eye are at the same level.) But the images are now by no means necessarily alternately erect and inverted, even though the curve of vertices be continuous. However, I merely note this extension of the rule, as we shall not require it in what follows.

I then investigated the form of the curve of vertices in a medium in which the square of the refractive index increases by a quantity proportional to the square of the distance from a plane in which it is a minimum, and found that (under special circumstances, not however possible in air) three images could be produced in such a medium. But the study of this case (which I could not easily explain here without the aid of mathematics) led me on as follows.

As the curvature of a ray is given by the ratio of the rate of change of index per unit of length perpendicular to the ray, to the index itself (a result which I find was at least virtually enunciated by Wollaston); and as all the rays producing the phenomena in question are very nearly horizontal:—*i.e.* perpendicular to the direction in which the refractive index changes most rapidly:—their curvatures are all practically the same at the same level. Hence if the rate of diminution of the refractive index, per foot of ascent, were nearly constant, through the part of the atmosphere in which the rays travel, the rays we need consider would all be approximately arcs of equal circles; and the curve of vertices would (so far as these rays are concerned) lean wholly *from* the eye; being, in fact, the inferior part of another equal circle which has its lowest point at the eye. Hence but one image, an erect one, would be formed; but it would be seen elevated above the true direction of the object. This is practically the ordinary horizontal refraction, so far as *terrestrial* objects on the horizon are concerned. The paths of the various rays would be of the form in Fig. 3 (the drawing

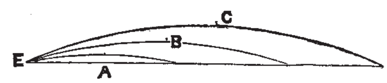

FIG. 3.

is, of course, immensely exaggerated) and the locus of vertices, A B C, obviously leans *from* the eye. But now suppose that, below a stratum of this kind, there were one of constant density, in which of course the rays would be straight lines. Then our sketch takes the form Fig. 4 (again exaggerated); each of the portions of the ray in

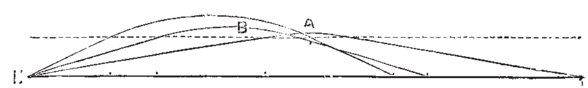

FIG. 4.

the upper medium being congruent to the corresponding one in the former figure (when the two figures are drawn to the same scale), but *pushed farther to the right as its extremities are less inclined to the horizon.* In its new form the curve of vertices A B C leans back *towards* the eye, and we have an inverted image. The lower medium need not be uniform as, for simplicity, we assumed above. All that is required is that the rate of diminution of density upwards shall be less in it than in the upper medium.

Those who have followed me so far will at once see that, as a more rapid decrease of density, commencing at a certain elevation, makes the curve of vertices lean back, so a less rapid decrease (tending to a "stationary state") at a still higher elevation will make the curve of vertices again lean forward from the eye. I need not enlarge upon this.

Thus to repeat:—the conditions requisite for the production of Vince's phenomenon, at least in the way conjectured by him, are, a stratum in which the refractive index diminishes upwards to a nearly stationary state, and below it a stratum in which the upward diminution is either less or vanishes altogether. The former condition secures the upper erect image, the latter the inverted image and the lower direct image.

In my paper read to the *Royal Society of Edinburgh* I have given the mathematical details following from the above statement; and have made full calculations for the effect of a transition stratum, such as must occur between two uniform strata of air of which the upper has the higher temperature. From Scoresby's remarks it appears almost certain that something like this was the state of affairs when the majority (at least) of his observations were made. When two masses of the same fluid, at different temperatures, rest in contact; or when two fluids of different refractive index, as brine and pure

water, diffuse into one another; the intervening layer must have a practically "stationary" refractive index at each of its bounding surfaces, and a stratum of greatest rate of change of index about midway between them. The exact law of change in the stratum is a matter of comparatively little consequence. I have assumed (after several trials) a simple harmonic law for the change of the square of the refractive index within the stratum. This satisfies all the above conditions, and thus cannot in any case be very far from the truth. But its special merit, and for my purpose this was invaluable, is that it leads to results which involve expressions easily calculated numerically by means of Legendre's Tables of Elliptic Integrals. This numerical work can be done once for all, and then we can introduce at leisure the most probable hypotheses as to the thickness of the transition stratum, the height of its lower surface above the ground, and the whole change of temperature in passing through it. I need not now give the details for more than one case, and I shall therefore select that of a transition stratum 50 feet thick, and commencing 50 feet above the ground. From the physical properties of air, and the observed fact that the utmost angular elevation of the observed images is not much more than a quarter of a degree, we find that the upper uniform layer of air must under the conditions assigned be about 7° C. warmer than the lower. Hence by the assumed law in the stratum, the maximum rise of temperature per foot of ascent (about the middle of the transition stratum) must be about 0°·2 C. per foot. Such changes have actually been observed by Glaisher in his balloon ascents, so that thus far the hypothesis is justified. But we have an independent means of testing it. The form of the curve of vertices is now somewhat like the full lines in the following cut, Fig. 5:—

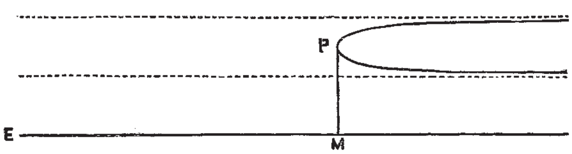

FIG. 5.

where E is the eye, and the dotted lines represent the boundaries of the transition stratum. It is clear that, if PM be the vertical tangent, there can be but one image of an object unless its distance from E is at least twice EM. This will therefore be called the "*Critical distance*." If the distance be greater than this there are three images:—one erect, seen directly through the lower uniform stratum—then an inverted one, due to the diminution of refractive index above the lower boundary of the transition stratum—and finally an erect image, due to the approximation to a stationary state towards the upper boundary of that stratum. Now calculation from our assumed data gives EM about six miles, so that the nearest objects affected should be about twelve miles off. Scoresby says that the usual distance was from ten to fifteen miles. Thus the hypothesis passes, with credit, this independent and severe test. A slight reduction of the assumed thickness of the transition-stratum, or of its height above the ground, would make the agreement exact.

All the phenomena described in Vince's paper of 1799, as well as a great many of those figured in Scoresby's works, can easily be explained by the above assumptions. Scoresby's remarkable observation of a single inverted image of his father's ship (when thirty miles off, and of course far below the horizon) requires merely a more rapid diminution of density at a definite height above the sea. His figure is the second in Fig. 1 above. But Scoresby figures, as above shown, several cases in which two or more inverted images, without corresponding erect ones, were seen above the ordinary direct image. The natural explanation is, of course, a series of horizontal layers of upward diminishing density and without a "stationary state" towards their upper bounding planes. I find that, by roughly stirring (for a very short time) a trough in which weak brine below is diffusing into pure water above, we can reproduce this phenomenon with great ease. In fact, when temporary equilibrium sets in, the fluids are arranged in a number of successive parallel strata with somewhat abrupt changes of density.

But the mathematical investigation, already spoken of, shows that it is quite possible that there may be layers tending to a stationary state without any corresponding *visible* images.

This depends on the fact that, while the inverted image (due to the lower part of a stratum) is *always* taller than the object seen directly (though not much taller unless the object is about the critical distance); the numerical calculation shows that the erect image is in general extremely small, and can come into notice only when the object is not far beyond the critical distance. Thus there may have been, in *all* of Scoresby's observations (though he has only occasionally noticed and depicted them) an erect image above each inverted one, but so much reduced in vertical height as to have been invisible in his telescope, or at least to have formed a mere horizontal line so narrow that it did not attract his attention. The greatly superior number of inverted images, compared with that of the direct ones, figured by Scoresby may thus be looked upon as another independent confirmation of the approximate correctness of the hypothetical arrangement we have been considering.

To obtain an experimental repetition of the phenomena in the manner indicated by the above hypothesis, a tank, with parallel glass ends, and about 4 feet long, was half-filled with weak brine (carefully filtered). Pure water was then cautiously introduced above it, till the tank was nearly filled. After a few hours the whole had settled down into a state of slow and steady diffusion, and Vince's phenomenon was beautifully shown. The object was a metal plate with a small hole in it, and a lamp with a porcelain globe was placed behind it. The hole was triangular, with one side horizontal (to allow of distinction between direct and inverted images), and was placed near one end of the tank, a little below the surface-level of the unaltered brine, the eye being in a corresponding position at the other end. A little vertical adjustment of object and eye was required from time to time as the diffusion progressed. The theoretical results that the upper erect image is usually much less than the object, and that it is seen by slowly convergent rays, while the inverted image is larger than the object and is seen by diverging rays, were easily verified.

To contrast Wollaston's best-known experiment with this, a narrow tank with parallel sides was half-filled with very strong brine, and then cautiously filled up with pure water. (The strong brine was employed to make up, as far as possible, for the shortened path of the rays in the transition stratum.) Phenomena somewhat resembling the former were now seen, when object and eye were nearly at the same distance apart as before, and the tank about half-way between them. But in this case the disparity of size between the images was not so marked—the upper erect image was always seen by diverging rays, the inverted image by rays diverging or converging according as the eye was withdrawn from, or made to approach, the tank. In this case, the curvature of each of the rays in the vessel is practically constant, but is greatest for the rays which pass most nearly through the stratum of most rapid change of refractive index. Hence, when a parallel beam of light fell horizontally on the tank and was received on a sufficiently distant screen, the lower boundary of the illuminated space was blue—and the progress of the diffusion could be watched with great precision by the gradual displacement of this blue band.

I propose to employ this arrangement for the measurement of the rate of diffusion, but for particulars I must refer to my forthcoming paper.

Wollaston's experiment with the red-hot poker was probably, his experiment with the long red-hot bar of iron almost certainly, similar to that above described with the long tank and the weak brine; and *not* to that with the short tank, though the latter is usually cited as Wollaston's contribution to the explanation of the Vince phenomenon. We have seen how essentially different they are, and that the latter does not correspond to the conditions presented in nature. P. G. TAIT

NOTES

THE Council of the Scottish Meteorological Society are soliciting subscriptions, however small, for the proposed Ben Nevis Observatory. It is essential to the success of this important national undertaking that the buildings should be erected during the present summer, and several thousand pounds are required before operations can be commenced. A considerable sum has already been received in liberal subscriptions from a few individuals, but not nearly enough for the purpose. We trust that many of our readers will send what they can to the Scottish Meteorological Society, Edinburgh.

DR. WILD, president of the International Circumpolar observation parties, announces that in conformity with the request of several Governments, the observations now going on round the Pole will not be prolonged beyond the time originally fixed, viz. September, and that all the parties, if not prevented by ice, will be back within that month.

A LETTER read at the Paris Geographical Society states that P. Vidal, French missionary to Samoa, has discovered the remains of La Perouse and his unfortunate companions.

THE Rev. S. J. Perry, S.J., has lately been elected a Corresponding Member of the Accademia dei Lincei.

DR. HENRY SCHLIEMANN has been elected an Honorary Fellow of Queen's College, Oxford.

LAST week we announced that a baronetcy had deservedly been conferred on Dr. William Chambers, and this week we regret to announce the death of the veteran publisher in his eighty-fourth year. As the head of the firm of Messrs. W. and R. Chambers, he has through a long life done splendid service in the spread of education, and of a knowledge of science. In his "Information for the People," his "Tracts," his text-books of science, among the first of their kind, and by various other means, he did good pioneer work in scientific literature and education.

IN reference to our note last week (p. 63), a correspondent writes that the American table at Naples is being used by its first occupant, Dr. E. B. Wilson, of the Johns Hopkins University, Baltimore. Dr. Wilson has been working during a part of the year at Cambridge on early mammalian embryology, and at Naples his work will probably be either on certain points in the development of some of the Cœlenterata or upon the embryology of the Dicegemidæ as available material permits. Williams College, Mass., which holds the American table, receives a brief course of lectures from each worker whom it appoints to the privileges of the Naples Station.

ON the evening of Friday last week several tornadoes swept over the states of Minnesota, Wisconsin, Illinois, and Missouri, which were exceptionally destructive to life and property even for that tornado-troubled region. It is reported that 83 persons have been killed and 340 injured, many of them fatally, and a very large number of houses reduced to ruins. Of these tornadoes the most terrible in its destructiveness would appear to have been the one which passed over Racine in the south-east of Wisconsin, killing 25 and injuring 100 persons, and wrecking 150 buildings. The path of the tornado was about 400 yards wide and half a mile long, and all buildings, particularly those in the central line of its path, collapsed into mere masses of ruins. Waggons and other movable articles were blown into Lake Michigan, over which the tornado passed on leaving the town, the whirling columns of clouds and the violent commotions of the lake presenting a grand and impressive spectacle. The recently published "Professional Papers of the Signal Service, No. VII." show that the region over which these tornadoes passed is comprehended within that portion of the United States where tornadoes are of most frequent occurrence.

MR. BRUNLEES, the President of the Institution of Civil Engineers, has sent out invitations for a *conversazione* at the South Kensington Museum on Wednesday, the 30th inst.

ON Saturday last, May 19, the Essex Field Club held its first meeting of the session. The party, nearly ninety in number, alighted at Theydon Bois Station on the Ongar branch of the Great Eastern Railway, and proceeded through Epping Forest to Ambresbury Banks, where they were met by Sir T. Fowell Buxton. The party was then conducted through the splendid park belonging to the Copt Hall Estate, and finally assembled at Warlies, Waltham Abbey, the seat of Sir Fowell Buxton, who had kindly invited the Club for the occasion. In the course of the evening a paper on "English Plant Names" was read by Mr. J. Britten, F.L.S.

THE Paris Aëronautical Exhibition will be opened at the Trocadéro on June 5 and close on the 18th. MM. Janssen, Berthelot, Paul Bert, and Hervi Mangon are among the members of the committee, as well as a number of senators and deputies. The festival will take place at Annonay on July 29, and statues of the two brothers Montgolfier will be erected on the public place of the city. A competition has been opened in Paris, and the works of competitors are on view at the Cercle de la Librairie. The jurymen, mostly members of the Academy of Beaux Arts, will give their award on Saturday next. The height of the monument and pedestal will be 7 metres. The prize is 3000 francs for the plaster model to be exhibited at Annonay on July 29, and 40,000 francs for the bronze. The marble for the pedestal will be given by Government. A public banquet will be given in Paris, M. Gaston Tissandier being in the chair.

ON April 29, at 10.30 p.m., a brilliant meteor was observed in Jondalen in Norway. It appeared in the east, and went in a southerly direction, where it passed out of sight. Its size to the eye was about the same as the moon's, while its shape appeared to be conic. The colour of its track was deep red, and it shone so brilliantly that the smallest objects could be seen on the ground. It lasted several seconds, and disappeared behind some mountains.

ON the 13th, at 8 o'clock in the evening, a large meteor was observed at Epinal, travelling from south-east to north-west; it had a disk which has been estimated at a decimeter. The tail was of a pinky colour; a noise from explosion was heard. It was also observed at Mulhausen.

THE Reports on the Public Gardens and Plantations in Jamaica are becoming yearly of more importance. That for the year ending September 30 last is now before us. Mr. Morris opens his report by bearing testimony to the liberality of the Steamship and Railway Companies in conveying plants free of charge to the different ports and railway stations. "By these means," it is stated, "districts, formerly beyond the reach of the Public Gardens, have been able to obtain plants as conveniently and as